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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Optimising predictor domains for spatially coherent precipitation downscaling" *by* S. Radanovics et al.

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Text in bold face correspond to the reviewer's comment, while the authors answers are in normal font.

This manuscript examines the way to define predictor domains used by an analogue downscaling method for precipitation downscaling for target zones covering France. The authors make a convincing analysis of the problem and their manuscript deserves publication in HESS.

The authors would like to thank the reviewer for the positive consideration of the manuscript.

However, I think some improvements (minor revision) could be implemented before publication.

SOME GENERAL COMMENTS.

1. In its present form, the work appears to be highly regional in focus and the fine details are unlikely to generalise to other locations or variables (e.g. predictand or predictors). However, the paper could have sufficiently broad geophysical implications. For example, the optimization methodology could be more fully explained (is it applicable everywhere?) and the possible ways to deal with this topic could be better clarified. Comments on this would be welcome.

The authors would like to thank the referee for this comment that led us to rewrite and reorganize the abstract, the introduction and the conclusions in order to distinguish the parts of this study that may have broader implications (development of the optimisation algorithm) from the more regionally-oriented conclusions. Comments on the applicability of the optimisation algorithm to other predictand, predictor or even downscaling methods have for example been added in the conclusions.

2. It would be appropriate to better clarify whether this manuscript focuses on precipitation downscaling in climate change context, in weather forecasting, or in both, discussing the assumptions and implications of the downscaling in both cases. For example, the rather complex downscaling method adopted (e.g. 4 steps, several predictors, etc.) is not parsimonious and may not to be robust under climate change conditions, while it could work in weather forecasting.

The downscaling procedure has originally been developed in a precipitation forecast context. One of the assumptions of the downscaling is that the large scale predictors are reasonably well simulated by the global models. For nowadays weather forecast models this is the case. Global climate models should give at least realistic geopotential- and temperature fields. The humidity variables are a bit more difficult, but we would argue that for precipitation downscaling it is important to have the information

on the available water in the atmosphere. The most questionable predictor variable in a climate change context is the vertical velocity that has been added because it led to fewer false alarms on the first two forecast days in the forecast context but is certainly not very well simulated by climate models. Therefore a version using three predictor variables would be preferable in the climate change context.

To answer the question of the robustness of the method under climate change conditions a detailed validation will be required. Like for every statistical downscaling method the assumption is that the relationship between predictors and predictands found for the present day conditions will not change under future conditions. With the predictor set chosen for the presented method the key large scale ingredients that lead to precipitation in the mid-latitudes are taken into account and these ingredients are supposed to be robust under climate change. The major problem of the analogue method in a climate change context is that it is not able to produce new extremes, so it is not the most appropriate tool for studying extremes. Since the analogue search is not restricted to specific seasons, it could to some degree adapt to a shift in seasons.

The present study is performed in a reanalysis context with a perfect prognosis method, so the application context is not the focus of this work. A sentence has been added in section 2.2 to specify that this method has been originally developed in a weather forecasting context and that developments are underway to apply it in a climate change context.

3. Please clarify what is meant by "spatially coherent precipitation" in this study? For example, if several analogue days are selected and aggregated together considering the mean of the analogues, the downscaled field loses its true spatial coherence, regardless of the domain(s) of the predictor(s) considered.

Spatially coherent precipitation means precipitation with a realistic spatial distribution. Using a summary measure like the mean of precipitation from all selected analog days or an other characteristic to describe the empirical distribution indeed destroys the

spatial and the temporal structure. For hydrological modelling timeseries are created from the output using only one analogue day per timestep. This definition has been added in the introduction.

4. Length of the paper. Although there are no restrictions on the number of words - at least as far as I know - the article in its current form is very dense and hard to follow. I therefore propose summarizing where possible.

The authors did their best to summarise some paragraphs but the total length of the paper could hardly be reduced due to some additional analysis suggested by the reviewers as well as the responses to their quite relevant comments.

SOME SPECIFIC COMMENTS.

1. TITLE. See general comment 3. As a suggestion: "over France" could be added to the title (see also general comment 1).

In agreement with the response to general comment 1 and specific comment 2 below, the title has not been changed.

2. ABSTRACT. The abstract should help to understand the key messages of this study (see general comment 1). Please reword and clarify the broad geophysical implications of this study.

The abstract has been reworded to hopefully better delivering the key messages, including the broader applicability of the optimisation algorithm.

3. P 4018 line 16. Add "weather" before "forecasting".

Changed as requested.

4. P 4019 lines 6-12. This could be false. See general comment 3, too.

It is true as long as only one date at a given time is selected but false for summary measures. A comment has been added to the text (section 1.2).

5. P 4020 line 9. Please correct “ressources".

Changed as requested.

6. P 4020 line 11. Horton et al. → (Horton et al. ...).

Changed as requested.

7. P 4020 line 14. Why only geopotential?

The optimisation was restricted to geopotential in order to be able to do the optimisation for a large number of target locations and to consider a few near-optimum domains rather than searching for a unique optimum following the equifinality thesis. Geopotential was chosen for optimisation because it is the most important predictor in this method and the size and location of the predictor domain is supposed to depend more strongly on the typical weather pattern causing precipitation in the target area than for the other predictor variables. This explanation has been added to the text in section 1.2.

8. P 4020 lines 15-17. Not clear.

Restricting the parameters to be optimised allows optimising domains for a large number of target zones separately and to obtain a few near-optimum domains for each target zone rather than searching for a unique optimum as suggested by the equifinality thesis. This explanation has been added to the text in section 1.2.

9. P 4020 lines 18-27. Please write the objective of the paper once only (already reported at lines 13-17 p 4019).

The introduction has been reorganized and now hopefully clearly shows the objectives of the paper.

10. P 4021 lines 14-15. What are the consequences of this?

The advanced data assimilation method allows to make better use of the available

observations, especially satellite data. This leads for example to better temporal consistency and a better representation of the hydrological cycle as described in Dee et al. (2011). A long archive is very important for the analogue method, because the analogue method can not create situations that are not in the archive. The longer the archive the more rare situations will be included. Changes were made in the text in section 2.1.1.

11. P 4022 line 17 (and abstract). “Relevance maps" is not standard terminology, please define.

Relevance maps are maps showing the skill of the downscaling method using an unitary sized domain at all possible locations. The definition has been added to the text.

12. P 4022 line 17. Remove one “the".

Changed as requested.

13. P 4022 line 27. Why Fig. 4 here? I think it is better to remove it.

Removed as requested.

14. P 4023 line 1. Two times “autumn".

The second “autumn" has been replaced with “this season".

15. P 4023 l 20. Why temp. at 925 hPa at D+1?

The pressure levels and the times at which the temperature predictor is taken was the result of an optimisation for this parameters done by Ben Daoud et al. (2011). This information has been added to the text in section 2.2.

16. P 4023 l 21. Is there always one and only one point as the closest to the target zone?

Theoretically there could be up to four equally close ones, but given the precision of the coordinates it is very unlikely that equal distances appear.

17. P 4024 l 5. Why 4 days and not more or fewer?

4 days has been chosen because it corresponds approximately to the maximum time a typical low-pressure system needs to pass over the target location. This has been specified in section 2.2.

18. P4025 l11-15. So is this the first application to the whole of France? Also, is there no optimization of the entire downscaling procedure to the whole of France?

Yes this is the first application of this particular downscaling procedure to the whole of France. The entire downscaling procedure has been optimised for the Saône basin and the Seine basin by Ben Daoud (2010) and in this study all parameters—except the predictor domains—correspond to those found for these basins. This has been specified in the revised text.

19. P 4026 l 9. Which years?

The $CRPS_{climatology}$ is calculated for the 01 August 1982 - 31 July 2002 period (except for the 44 years experiments wher it is calculated over the whole 44 years). The time period has been added to the text in section 2.3.

20. Discussion. This part could be summarised and shifted to the previous sections.

The discussion section includes critical methodological choices that deserve some comments according to the authors, and these can hardly been summarized. This section has therefore been only slightly modified and kept as an independent section.

21. P 4028 RESULTS. Please add a comment on the size of the domains found. Is it greater or smaller than those normally used in precipitation downscaling? Probably it changes depending on the time scale considered.

The authors would like to thank the reviewer for this interesting comment. The domain

sizes used in other downscaling studies were compared to the domain sizes found in this study. Bontron (2004), Ben Daoud (2010), Timbal and McAvaney (2001), Boé et al. (2006) and Guilbaud and Obled (1998) used predictor domains with sizes of 20-25 degrees longitude and 10-15 degrees latitude which corresponds to upper-medium sized domains found in this study. Timbal and McAvaney (2001) (for daily minimum and maximum temperatures) tested somewhat smaller and much larger domains as well but found the 20 $\times$ 12 degrees one to perform best. The domains tested by Brigode et al. (2012) correspond to small to medium sized ones found in this study. Timbal et al. (2003) (for daily minimum and maximum temperatures) used a domain somewhat larger in north-south direction. Larger domains were used by Boé and Terray (2008), Hanssen-Bauer et al. (2003), Matulla et al. (2008) and Obled et al. (2002). All the above studies, as well as this work, considered a daily time scale for precipitation or—in the case of Timbal and McAvaney (2001); Timbal et al. (2003)—minimum and maximum temperatures. A paragraph has been added in section 3.2.3.

22. RESULTS. Did you try to compare the skill using optimized domains and those used in other studies (or with domains of different sizes)? In general, what is the value added to optimize the domain?

An additional experiment has been performed to answer this comment as well as similar ones from other reviewers. Predictor domains were optimised using the average precipitation over France. The starting point and the predictor domains for the other variables were chosen close to the centroid of France. The CRPSS for the best of the individually optimised domains at each zone is now compared with the CRPSS for the best domain optimised for the average precipitation over the whole country. The CRPSS for the optimised domains is between 0.4% - 77.7% better than the CRPSS for a domain optimised for the mean precipitation over the whole country depending on the location. The mean CRPSS over the whole country is 8.3% better. The new figure 5 now shows the CRPSS obtained with the individually optimised domains as well as the CRPSS using the best domain found for the mean precipitation. A paragraph

describing this additional experiment and related results has been added in section 3.2.1.

23. P 4036 lines 17-20. Not clear. What is meant by “data quality" in this context?

In the cited article (Timbal et al., 2003) data quality refers on one hand to the quality of the observation data in terms of homogeneity and on the other hand to the reliability of the reanalysis data used. Due to large amounts of satellite data that are assimilated during the last years of the reanalysis, it became more reliable especially over the oceans where there are not so many radiosonde stations. This has been clarified in the revised text.

24. P 4038 lines 3-18. This part could be summarized.

The conclusions have been reorganized and summarized.

25. P 4039 lines 3-5. You cannot generalize from a single example analyzed.

This comment should be related to P4040 L3-5. We have now modified the sentence to suggest this hypothesis.

26. P 4039 This part could be summarized.

cf. response to comment 24, the conclusions have been reorganized and summarized.

27. Fig 1. Please highlight the seven zones (e.g. with arrows).

The figure has been changed for hopefully better highlighting the case study zones.

28. Figures 7 and 8. Enlarge the coordinates.

Changed as requested.

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